University of Michigan Retirement Research Center

Working Paper
WP 2002-027

Wealth Shocks and Retirement Timing: Evidence from the Nineties

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MRRC

Project #: UM00-D1

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April 2002

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Acknowledgements

This work was supported by a grant from the Social Security Administration through the Michigan Retirement Research Center (Grant # 10-P-98358-5). The opinions and conclusions are solely those of the authors and should not be considered as representing the opinions or policy of the Social Security Administration or any agency of the Federal Government.

Wealth Shock and Retirement Timing: Evidence form the Nineties

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Abstract

This paper explores whether the timing of retirement responds to unexpected changes in wealth. Although the normality of leisure is a standard assumption in economic models, econometric support for it has not been consistent. The period of the 1990s allows a reexamination of this question because of the large and unexpected capital gains realized by many households. Using the 1992 to 1998 waves of the Health and Retirement Study, and two different identification strategies, I find evidence consistent with the theoretical expectations of wealth effects. Difference-in-differences estimates suggest that a $50,000 wealth shock would lead to a 1.9 percentage point increase in retirement probability among individuals ages 55 to 60. Estimates using panel data on savings and wealth find the elasticity of retirement flows between 1996 and 1998 with respect to wealth is between 0.39 and 0.50 for men.

Author's Acknowledgement

This paper is based on Chapter I of my dissertation. I am grateful to the Michigan Retirement Research Center, the Institute for Social Research, and the Social Security Administration for funding. The paper has benefited from feedback from Charlie Brown, Kerwin Charles, Julie Cullen, Gary Engelhardt, Joe Lupton, Lucie Schmidt, Matthew Shapiro, Bob Willis and seminar participants at the University of Michigan Labor Seminar. All remaining errors are my own.

Do increases in wealth cause people to retire? It is difficult to answer to this question because, as the life-cycle model illustrates, one of the major reasons for household saving is retirement. Thus to estimate the effects of wealth, it is necessary to find some exogenous variation in wealth across households. In this paper, I exploit the bull market of the 1990s to study the effect of wealth on retirement timing. The unprecedented price appreciation of public equity markets in the period blessed many households with extraordinary capital gains that were both large and unexpected. In this paper, I use panel data on wealth, saving, and portfolio allocation from 1992 to 1998 to exploit variation in the *unexpected* component of the wealth gains to test for wealth effects. I find evidence that unexpected capital gains significantly increase the probability of retirement for men.[1] This finding is robust to controls for variation in individuals' baseline expected retirement ages.

The 1990s is a particularly useful period for the study of wealth effects because the gains in the nineties were so large, that even with reporting error, a great deal of wealth variation is identifiable. In addition, because the gains in the nineties were much larger than expected, a smaller share of the variation in wealth will be due to endogenous variation in savings behavior. The timing of this phenomenon coincided perfectly with the start of the longitudinal Health and Retirement Study (HRS). Every two years, the Study collects data on wealth, labor supply, health and family, from individuals ages 50 and older. In this paper, I use data from the first four waves of the HRS, starting in 1992, before the stock market run-up, through 1998.

The paper starts with a discussion of the importance of understanding wealth effects on retirement. I follow with a review of the challenges in estimating wealth effects because much of the variation in wealth is endogenous. I then present a simple theoretical framework for studying the effect of wealth on retirement timing. I show that a wealth shock will reduce the incentive for continued work because the marginal utility of consumption has decreased. Using the panel data in the HRS on income,

[1] Although the current paper is focusing on the period of positive gains in the 1990s, as data from the 2000 and 2002 Health and Retirement Study become available, I will extend this research to examine retirement responses to the stock market declines since 2000. In addition, it will be interesting to examine subsequent labor supply and consumption responses of those who had already retired before the downturn.

wealth, and saving, I then create measures of sustainable retirement consumption both before and after the wealth shock, that capture variation in the changes in retirement incentives due to the wealth shock, as suggested by the theory. I estimate that the elasticity of retirement flows between 1996 and 1998 with respect to wealth is between 0.39 and 0.50 for men. To control for the fact that the incidence of the shocks may not be exogenous (i.e. a household must own stocks to have a wealth shock from market fluctuations) I control for baseline wealth and ownership of various types of assets. In addition, I show that baseline portfolio allocation is not related to unobserved determinants of retirement plans and that the estimated wealth effects are robust when controlling for baseline expected retirement age.

I then develop an alternative strategy to address the fact that the incidence of the shocks may not be exogenous -- a difference-in-differences test. I compare differences in retirement rates among individuals with DC pension plans between 1992 and 1998 to differences in retirement rates among individuals with DB pension plans between 1992 and 1998. Like the micro data analysis, this methodology isolates exogenous variation in wealth. I find a sharp increase in the retirement rate among men with DC pensions over the period, but no increase in retirement among men with other pensions. Consistent with the first set of results, the difference-in-differences results suggest men who likely had large windfalls in their pension wealth – those with DC pensions in 1998 – retire earlier than other men.

The Importance of Understanding Wealth Effects on Retirement

The way in which Americans save for retirement has fundamentally changed over the past 25 years. This change is characterized by an increase in risk in retirement resources. The responsiveness of retirement timing to wealth shocks is of increasing interest as a greater proportion of retirement resources are subject to fluctuation because of this risk. The effect of this added uncertainty on savings behavior and retirement timing is of its own interest and will not be addressed here.[2] However, because it can be

[2] See Burtless, Gary, "How Would Financial Risk Affect Retirement Income Under Individual Accounts? " *An Issue in Brief; Center for Retirement Research at Boston College* (5) (2000), and Samwick A. and Skinner J. “How Will Defined Contribution Pension Plans Affect Retirement Income?” *NBER Working Paper #6645*, 1998.

responsible for large unexpected fluctuations in wealth, the increase in uncertainty is a motivation for the study of wealth effects.

The growth of defined contribution (DC) pension plans relative to defined benefit (DB) pension plans accounts for much of this change. DB pension plans (similar to Social Security), are often thought of as "traditional" pension plans. Upon retirement from a firm, workers receive a guaranteed (at least nominally) pension payment based on their years of service and salary, for a fixed number of years or until death. Many such pensions have been replaced by DC pensions, which are stocks of wealth rather than flows. The most common of these is a 401(k) plan, in which plan balances are invested in assets of the worker's choice. The stock of assets available upon retirement depends on the amount workers and their employers contributed during the workers' tenure, the workers' portfolio choice, and the returns on the assets in portfolios. Whereas employers bear the financial risk in DB plans, workers bear the financial risk in DC plans. As Figure 1.1 shows, between 1980 and 1993, the percent of pension holders with DC plans increased from 34 percent to 52 percent (EBRI, 1997).

In addition to the growth of DC pensions, the share of households investing in risky assets outside of pensions – through mutual funds or direct ownership of stocks – has increased. In 1998, there were 6.8 million more households holding stock directly, and 17.4 million more household holding stocks through mutual funds than there were in 1989 (Poterba, 2001). This translates into an increase from 32 percent of households to 49 percent of households holding stocks in taxable (non-retirement) accounts. These increases were seen across age, demographic and socio-economic groups.

The share of retirement resources in risky assets may increase further if any of a variety of Social Security "privatization" measures is passed. A primary motivation for private accounts is that individuals could have greater Social Security wealth at retirement by contributing less than they currently do, because of the high expected returns in the stock market.

Although individual accounts may increase Social Security wealth, and the shift from DB to DC pensions may increase pension wealth, these changes will continue to add substantial uncertainty to retirement resources. For example, from simulations, Burtless (2000) estimates that the replacement rate for a worker with a DC pension fully invested in stocks who retired in 1969 would be 100% of peak earnings, while the rate for a 1975 retiree with an identical work history and investment strategy would be only 42%. Feldstein and Ranguelova's (2001) simulations for an individual account with a 4 percent contribution rate throughout one's working years, yield a distribution of Social Security benefits that range from 26 percent of current benefits to over 10 times that of current benefits.

As a greater share of retirement resources is invested in risky assets, the probability that an individual nears their planned retirement age with substantially greater or lower wealth than expected increases. Although the decline in male labor force participation rates at older ages witnessed in the last century appears to have leveled off in recent years (Anderson, Gustman, and Steinmeier, 1999), deviations in retirement wealth from expected retirement wealth may lead to transitory changes in retirement timing. Economic theory predicts that some of a wealth shock should be "consumed" by changes in leisure. On the other hand, norms to retire at age 62 or age 65 may be so strong that they may dominate any wealth effect, in which case deviations from expected wealth will just be absorbed by deviations in consumption or bequests.

Empirical Challenges and Approaches in Estimating Wealth Effects

Wealth effects are difficult to identify among individuals of any age, because wealth is not randomly assigned. Some of the variation in wealth reflects individual heterogeneity in preferences that will also be correlated with labor supply decisions. In the context of retirement behavior, high wealth could be a *result* of plans to retire early. All else equal, an individual who plans to retire sooner than another individual should be saving more during her working years, because she will have more years in retirement during which to live off of her accumulated wealth. Alternatively, high wealth individuals may have strong tastes for work and thus retire later. Thus, cross-sectional estimates of the effect of wealth on retirement timing could be biased upward or downward. This is illustrated in Table 1, which contains mean wealth among retired and non-retired men, as reported biennially in the HRS from 1992 to

1998. The sign of the difference in wealth is not consistent across years. Furthermore, when controlling for other covariates that effect retirement, wealth is not a significant predictor of retirement status, as indicated by the high p-values in the last column. Panel data models estimating the effect of changes in wealth on changes in planned retirement dates would also be biased because some portion of the changes in wealth would reflect endogenous savings behavior.

The early literature suggests such a bias. Studies often used measures of unearned income, such as wife's earnings, income from dividends, rent, and government programs, to estimate the effect of wealth on labor supply (Kosters, 1966; Ashenfelter and Heckman, 1973), but they are clearly not exogenous to the labor supply of the husband in the household. Although voluminous, the literature has failed to find consistent evidence of wealth effects. In his survey of men's labor supply, Pencavel (1986) writes, "of the 57 different estimated coefficients on net worth…only 16 would be judged as significantly different from zero...of these 16, exactly one-half is positive and one-half is negative...this hardly constitutes a resounding corroboration of the conventional static model of labor supply."

A number of more recent papers have examined wealth effects due to arguably exogenous policy changes, but continue to provide mixed results. Hurd and Boskin (1984) find that the Social Security benefit increases from 1969 to 1972 can explain a large amount of the acceleration of retirement in that period, whereas Burtless (1986) using the same data finds that the effects were very small. Krueger and Pischke (1992) estimate the effect of reductions in benefits due to amendments to Social Security in 1977 and find no effect. A more recent study by Chan and Stevens (2000) finds that individuals' planned retirement ages do respond to perceived changes in pensions.

Several recent studies avoid the problem of endogenous wealth variation by making use of natural experiments. Imbens, Rubin, and Sacerdote (2001) survey lottery winners and find significant labor supply effects of winnings, particularly among individuals ages 55 to 65. A recent paper by Kimball and Shapiro (2001) making use of survey questions on hypothetical lottery winnings finds large responses to wealth gains. Holtz-Eakin, Joulfaian and Rosen (1993) find that individuals in households that receive large inheritances are more likely to leave the labor force and less likely to enter the labor force.

There is strong evidence that an individual's retirement timing responds to *expected* changes in wealth. Accrual rates in Social Security (Coile and Gruber, 2000) and private defined benefit pension plans (Stock and Wise, 1990; Samwick, 1998) have been found to be big determinants of retirement timing. Although these effects are not referred to as wealth effects, they provide evidence of the sensitivity of retirement timing to the path of wealth accumulation.

Even if the endogeneity problem can be surmounted by use of natural experiments, estimation is complicated by measurement error in survey data on wealth. Survey measures of wealth are notoriously poor (Curtin et al., 1989). Respondents may refuse to answer questions about their assets or they simply may not know the exact values of the assets. This can be particularly problematic in studies of wealth effects that require calculating *changes in wealth*. Unless measurement error is perfectly correlated across time periods, changes in wealth will be measured with even greater error. Analyses of such data have found that this is indeed the case (Juster et al., 1999).

The economic boom of the 1990s allows a unique reexamination of this question. One of most discussed phenomena of the late nineties is the extraordinary performance of the stock market. Figure 1.2, which plots the year-end level of the Standard and Poor 500 Index, illustrates the gains made after 1994. Cheng and French (2000) estimate that for every dollar invested in the stock market in 1994, an individual received $1.12 of unexpected gain, in addition to a $0.70 expected gain by the end of 1999.

At the same time that the stock market was booming, however, labor force participation rates among those close to retirement age may have been increasing.[3] Thus, recent time series data on wealth and labor force participation provide no evidence that wealth shocks covary with withdrawal from the labor force. These aggregate data likely reflect that there are many other determinants of labor force

[3] See for example Blank, Rebecca M. and Matthew D. Shapiro. "Labor and the Sustainability of Output and Productivity Growth," in *The Roaring Nineties: Can Full Employment Be Sustained?*, Alan B. Krueger and Robert M. Solow, eds. Russell Sage Foundation/New Century Foundation, New York: Russell Sage Foundation, 2002.

participation – including retirement age norms, changes in health, changes in Social Security incentives, and changes in wages driven by local labor market conditions. National unemployment rates, which were decreasing almost constantly from a high in June of 1992 of 7.8 percent to a low of 3.9 percent in September of 2000,[4] likely led to increases in demand, and thus higher wages, for older workers.

It is possible that even though overall retirement rates were declining, they may have been increasing for those individuals who had financial windfalls. Suggestive evidence of this is found in Coronado and Perozek (2001) who find that households that held corporate equity in the early nineties were more likely to retire earlier than expected. In this paper, I isolate exogenous variation in wealth due to the capital gains in the 1990s in two alternative ways using the Health and Retirement Study (HRS). Particularly useful for this study is the data in HRS on assets and active savings and dissaving of assets. The wealth data in the HRS may be considerably better than the data in earlier surveys because of the use of innovative interviewing techniques, such as follow-up brackets for non-response (Juster and Smith, 1997). Also useful is that although the HRS is a panel data set, a new cohort of respondents was "aged in" to the sample in 1998, allowing use of cross-cohort comparisons as the second empirical approach in the paper. Detailed descriptions of the samples used in the paper are in the empirical sections below.

Why do Individuals Retire?

To assess the role of wealth shocks on retirement timing, it is useful to consider a life-cycle model of retirement. I present a simple model with no uncertainty that is intended to serve as a benchmark from which to understand the main effects of wealth on the retirement decision.[5] The assumptions of the model are the following: individuals gain utility from leisure and the consumption of market goods and services; work is a discrete decision and individuals work for all periods $t<R$ where R is the date of retirement; at retirement, they fully annuitize all of their wealth via a fairly priced annuity

[4] As reported on the Bureau of Labor Statistics website http://www.bls.gov.
[5] The only source of uncertainty is the age of death, and it has no effect because individuals are assumed to purchase fairly priced annuities at the time of retirement.

market,[6] and follow a smooth consumption profile until death. Because all of their wealth is annuitized, they leave no bequests.[7]

Individuals maximize the expectation of a lifetime utility function of the form:

$$(1)\ V_t = E_t \sum_{s=t}^{S} \beta^{s-t}\, u_s(c_s, l_s)$$

where $u_s(\bullet)$ is the instantaneous utility function, S is the year by which the individual is certain to have died, c is consumption, l is leisure, β is the discount factor. Utility from consumption and leisure may change as the individual ages, because of factors such as deteriorating health. They face a lifetime budget constraint of the form:

$$(2)\ \sum_{s=t}^{S}\left(\frac{1}{1+r}\right)^{s-t} c_s = A_t + \sum_{s=t}^{S}\left(\frac{1}{1+r}\right)^{s-t} y_s(R)$$

which simply states that the present value of the consumption in all the remaining years of life must equal the sum of current assets and the sum of the present value of income in all the remaining years of life. A_t is the value of all assets at time t and r is the real interest rate. Income, y, includes labor earnings, Social Security benefits, DB pension benefits, and income from assets, and thus its value in each period depends on the date of retirement R.

Workers will pick their retirement date R to maximize remaining lifetime utility.[8] For simplicity, because workers are not choosing among a continuous distribution of hours, we can say that at $t<R$, l=0 and at $t \geq R$ l=1. The gain to lifetime utility from continuing to work another year comes from the

[6] Research on the private annuities market suggests this is a reasonable assumption. Mitchell, Poterba, and Warshawsky (1999) find that the “money’s worth” of annuities has increased over the past decade and that at current prices and reasonable estimates of behavioral parameters, retirees should value annuities. Furthermore, as Kotlikoff and Spivak (1981) show, the family can almost perfectly substitute for a private annuities market.
[7] This is a simplifying assumption and adding an exogenous bequest does not change the implications of the model.
[8] There is a growing literature on the joint retirement decisions of spouses (Blau, 1998; Coile 2001; Hurd, 1990; Maestas 2001; Gustman and Steinmeier, 2001). A model incorporating this is beyond the scope of this paper. Empirically, as long as preferences for joint retirement are orthogonal to exogenous wealth shocks, this simplification will not bias my results.

resulting increase in lifetime consumption. The cost to lifetime utility from delaying retirement comes from the lost year of leisure.

A measure of the utility incentive to continue working that has been found to be empirically powerful is the “option value” of continued work originally developed by Stock and Wise (1990) and used in modified forms by many recent papers modeling retirement (Coile and Gruber, 2000; Chan and Stevens, 1999). In general, the option value is the difference between the lifetime utility associated with retiring at the optimal retirement date, R^*, and that associated with retiring at time *t*:

$$(3)\ OV_t = V_t(R^*) - V_t(R = t)$$

Workers should retire when the option value changes from positive to zero – that is when the utility gain from added lifetime consumption allowable from an extra year of work no longer exceeds the utility value of an extra year of leisure.[9] Empirically, we expect that the greater is OV_t, the less likely is retirement at *t*.

Although the simple life-cycle model presented here does not model uncertainty, it can still illustrate the effect of a wealth shock. A windfall in *A* will change the gain to working in a direct way. To see this, consider what a wealth shock does to consumption, holding labor supply constant. Given the assumption of complete annuity markets and smoothed retirement consumption, retirement consumption can be written as:

$$(4)\ c_{ret} = \frac{A_t + \sum_{s=t}^{S} \left(\frac{1}{1+r}\right)^{s-t} y_s(R)}{\sum_{s=t}^{S} \left(\frac{1}{1+r}\right)^{s-t}}$$

[9] The term “option value” is used in the literature to reflect that at retirement, workers give up the option to retire at a later date, which may have been more advantageous. Even in this model with no explicit uncertainty aside from mortality, it is an appropriate concept because when workers retire they lose the option of working with the payoffs they had at their pre-retirement job. That is, it is costly to return to work at a later date, because of losses associated with leaving a job - pension accrual, a particular wage, etc.

from the budget constraint (2). If individuals do not adjust their age of retirement, R, a dollar windfall increases sustainable consumption by $\dfrac{1}{\sum_{s=t}^{S}\left(\frac{1}{1+r}\right)^{s-t}}$.

In reality, individuals can adjust both consumption and the age of retirement in response to a windfall. Barring institutional constraints, if both goods are normal, a windfall will lead to increases in consumption and leisure. Because the increased utility from continued work comes from the resulting increase in lifetime consumption, as long as the marginal utility of consumption is declining (i.e. $u''(c) < 0$), a wealth shock will reduce the option value of continued work. In particular, the greater the wealth shock relative to the individual's planned retirement consumption, the greater should be the decrease in the option value of working. Thus, the probability of retirement over a fixed period of time should be increasing with the windfall.

In the next section, I calculate sustainable annual retirement consumption, contingent on retirement year, for a sample of workers in the HRS. I measure the magnitude of wealth shocks between 1992 and 1998 by how these shocks change this sustainable consumption level. In theory, if data are available on earnings and assets, one can impose a functional form for utility and calculate the change in the option value of continued work due to the wealth shock. However, these estimates would have no easily quantifiable interpretation. By measuring the reduced value of continued work in consumption units, I can easily interpret the estimated effects of the wealth shocks on retirement probabilities.

Wealth Effects Using Survey Measures of Capital Gains

The theory suggests that individual retirement timing should reflect variation in wealth. As discussed earlier, estimation of this relationship is complicated by the fact that much of wealth at retirement is expected and is a function of endogenous labor supply and savings decisions. The panel nature of the HRS allows a decomposition of wealth into expected and unexpected components. Because household wealth and savings behavior are observed at two-year intervals, wealth observed in later waves can be broken into endogenous wealth levels at baseline, endogenous savings since baseline, and capital gains since baseline. Because capital gains vary systematically by asset, they partly reflect individual choice over portfolio composition, and thus have an expected and unexpected component. I make some simple assumptions about expectations of capital gains to isolate the *unexpected capital gain* and assess its effect on retirement. Expected returns are based on historical trends as reported by Ibbotson Associates.[10] This method is discussed in detail after a description of the data.

Data

In this section of the paper, I use a sample of age-eligible men and women who are in the original Health and Retirement Study sample in 1992 and who did not attrite by 1998.[11] They are ages 57 to 67 in 1998. I focus on retirement transitions between 1996 and 1998 among individuals who were not retired in 1996. This is a strong selection criterion, and I limit the analysis to transitions during this short period so that I can decompose wealth into baseline wealth and shocks over as long a time period as possible – six years, given the data available. The resulting sample size is 1,972 for men and 1,890 for women. The sample is further reduced to 1,837 men and 1,726 women because of missing data.[12]

In each wave, the HRS asks a designated "financial respondent" in each household to report most of the financial information obtained by the survey. This includes values of and income from various assets, including housing, real estate, stocks, bonds, checking and savings accounts, and individual retirement accounts (IRAs). In addition, they report whether the household has added to or reduced the value of the following assets: housing, real estate, stocks, and IRAs. Each respondent who is currently working is asked to report characteristics of any pension plans at their current job. In addition, the HRS asks about pensions at jobs left since the last interview, and up to three prior jobs for respondents

[10] I used trends reported at http://www.troweprice.com/retirement/historical.html and http://www.investorhome.com/history.htm, but detailed reports are available at http://www.ibbotson.com.
[11] When weighted, the HRS is representative of individuals born between the years 1931 and 1941 living in the U.S. However it also surveyed respondents' spouses who may not have been of this cohort. Those born between 1931 and 1941 are "age eligible."
[12] Much of the missing wealth data has been imputed by HRS staff. However, individuals in households that refused to answer any wealth questions and individuals who did not report their earnings are excluded from the analysis.

interviewed for the first time. I make use of self-reported DC pension balances, contribution rates by the respondent, and matching rates of the employer.

The HRS asks respondents' permission to obtain their earnings from the Social Security Administration. I use this administrative data and a program similar to the Social Security Administration's *ANYPIA*[13] program to project benefits contingent on alternative retirement ages. For the quarter of the sample that did not release their SSA records to HRS, I impute Social Security benefits, using self-reported measures of earnings, age, and years of experience. For DB pension benefits, I also use administrative data obtained from respondents' employers. I used the HRS Pension Calculator Program (Curtin, 1998) to project pension benefits contingent on alternative retirement ages.

Table 2 contains summary statistics of wealth and unexpected capital gains as reported in the 1992 to 1998 HRS. Although the unit of analysis is the individual, the wealth measures are at the household level and in 1992 dollars. Total wealth is defined as the sum of housing, real estate and businesses, stocks, bonds, CDs, bank account balances, IRAs, and DC pension balances, less debts. Mean wealth for men in 1998 is about $477,000 and for women about $306,000. Overall, as a percent of wealth, capital gains were greatest during the 1996 to 1998 period. However, this pattern is not consistent across assets. Among stocks, they were greatest during the 1994 to 1996 period for men, and among DC pensions they were almost always negative. The negative capital gains in DC pension balances are not consistent with market patterns, and is perhaps indicative of reporting error in the data. I discuss reasons for this later in the paper.

Table 3 contains summary statistics for other variables that will be used in the analysis. The dummy variables for asset ownership show there is a great deal of variation in asset holdings among the sample. While about 85 percent are homeowners, less than 10 percent own bonds, and about a third hold stocks. About 45 percent of the sample has an IRA. The retirement transition rate between 1996 and 1998, based on the respondent's identification as being retired, is 17.7 percent for men and 15.3 percent

[13] See http://www.ssa.gov/retire2/anypia1.htm for a detailed description of *ANYPIA*.

for women. Substantially more men are married than women. About 18 percent of all individuals report being in fair or poor health. On average, men and women have 12-13 years of education, but men have about 10 years more work experience, and they earn about twice as much as women. About half of men and women have retiree health coverage from their employer or their spouse's employer.

Measuring Sustainable Consumption and Shocks

Because different households will value wealth shocks of equal size differently, due to variation in the stock of their wealth, it is important to control for these resources when estimating wealth effects. Retirement resources are not just stocks of savings, but also flows – Social Security, DB pension payments, and private annuity payments. Thus, it is convenient to convert all resources into either flows or stocks. In the analysis that follows, I convert all resources into flows, by calculating the annuity value of all household wealth and wealth shocks. This annuity value of wealth can be thought of as a proxy for retirement consumption.

Just as wealth can be broken into expected and unexpected components, so can these annuitized measures of wealth. Focusing on the HRS survey period, retirement consumption conditional on retirement in 1998, C_{98}, can be rewritten as:

$$(5)\quad C_{98} = {}_{92}C_{98} + (C_{98} - {}_{92}C_{98})$$

where ${}_{92}C_{98}$ is the 1992 expectation of retirement consumption. It is a function of wealth accumulated by 1992, savings between 1992 and 1998, and expected capital gains between 1992 and 1998. The second component, $(C_{98} - {}_{92}C_{98})$, represents that consumption due to unexpected capital gains between 1992 and 1998. Using the notation ${}_{t}\Delta_{t+1}C_{R}$ to represent the change between *t* and *(t+1)* in expectations of retirement consumption conditional on retirement at *R*, (5) can be broken into:

$$(6)\quad C_{98} = {}_{92}C_{98} + \left[\left({}_{92}\Delta_{94}C_{98} \right) + \left({}_{94}\Delta_{96}C_{98} \right) + \left({}_{96}\Delta_{98}C_{98} \right) \right]$$

which shows that retirement consumption is the sum of past expectations and current and lagged changes in expectations. Recall from the discussion of the theory, that it is these shocks to sustainable consumption that change the option value of retirement.

I estimate the level of potential retirement consumption from survey measures of household resources by assuming that individuals plan to smooth their post-retirement consumption. It is funded by Social Security (*ss*), DB pension payments (*p*), private annuity payments, and dissaving of private wealth (including the stock of DC pension wealth):

(7) $c_{ret} = ss + p + annuities + dissaving$

While Social Security benefits, DB pension payments, and annuity payments are directly observed or estimated using the survey data, dissaving must be calculated in a more complicated way. Following the assumptions laid out in earlier, individuals smooth consumption. To do so, those who retire before the age of pension receipt, a_p, or the age of Social Security receipt (age 62),[14] will dissave more in years until these payments begin. In a fair annuities market, individuals purchase real risk-free annuities at the time they retire (i.e. I assume a constant rate of interest across individuals and over time) that allow these differential rates of dissaving.[15] Thus, they die with neither assets nor debts. I use life tables by sex and race for survival probabilities needed to calculate dissaving and the annuity values of wealth. For simplicity, I assume the probability of living, P is zero after age 100. Thus, wealth at the age of retirement W_{a_r}, can be broken into three types of dissaving:

(8) $$W_{a_r} = p\sum_{i=a_r}^{a_p} P_i\left(\frac{1}{1+r}\right)^{i-a_r} + ss\sum_{i=a_r}^{61} P_i\left(\frac{1}{1+r}\right)^{i-a_r} + dissav\sum_{i=a_r}^{100} P_i\left(\frac{1}{1+r}\right)^{i-a_r}$$

The third term represents a constant amount that is dissaved between retirement and death; the second term represents additional dissaving between the year the individual retires and the year in which Social

[14] In reality, the timing of Social Security claiming need not coincide with the timing of retirement. I assume they do here for simplicity.
[15] See footnote 7 for a discussion of this assumption.

Security benefits begin; the first term represents additional dissaving between the year the individual retires and the year in which DB pension payments begin. Because some individuals have insufficient assets to smooth consumption until receipt of Social Security or pensions, I allow them to borrow at a rate of 18 percent to smooth consumption until then.[16]

From (8), I solve for *dissav* and add it to the other components of retirement consumption in (7) to obtain a measure for a constant level of retirement consumption conditional on retirement at a_r:

$$(9)\ c_{ret} = ss + p + annuities + \frac{W_{a_r} - ss\sum_{i=a_r}^{61} P_i\left(\frac{1}{1+r}\right)^{i-a_r} - p\sum_{i=a_r}^{a_p} P_i\left(\frac{1}{1+r}\right)^{i-a_r}}{\sum_{i=a_r}^{100} P_i\left(\frac{1}{1+r}\right)^{i-a_r}}$$

As described earlier, much of the variation in this sustainable retirement consumption level is due to savings decisions throughout an individual's lifetime. To estimate the effect of a wealth shock, c_{ret} must be broken up into expected and unexpected components, as in (6). In (9)**,** the shocks are part of W_{a_r}. They are calculated from the difference between wealth observed in two periods, as the residual after adjusting for the observed active saving or dissaving, and an expected rate of return. Active saving is observed for stocks, IRAs, DC pensions, housing, and real estate and businesses. I assume that all changes in the value of other assets are expected (the combination of savings and expected returns). Here I isolate the unanticipated capital gains in the HRS data for stocks, IRAs, and DC pensions.[17] I annuitize these unexpected capital gains for each asset and time period to create

$$(10)\ \left({}_t\Delta_{t+1} C_{98}\right)_j = \frac{W_{j,t+1} - (1+E_t r_{j,t})W_{j,t} - S_{j,t-(t+1)}}{\sum_{i=a_r}^{100}\left(\frac{1}{1+r}\right)^{i-a_r}}$$

[16] This high rate is used because it is in the range of easily obtainable credit cards.
[17] I have also done the analysis (not reported here for brevity) including capital gains in housing and real estate which are not found to be important.

the consumption value of a wealth shock between t and $t+1$ for asset j, conditional on retirement in 1998. $E_t r_{j,t}$ is the expected rate of return on asset j in time t, thus $(1+E_t r_{j,t})W_{j,t}$ is the expected capital gain. I use the two components of retirement consumption – that due to expected wealth and that due to unexpected wealth – in a regression analysis of retirement.[18]

First, I normalize the measures by pre-retirement consumption. When making retirement decisions for any given year, households may compare the consumption level they could maintain in retirement, conditional on retirement in that year, to their level of consumption before retirement. Thus, a $10,000 shock may mean one thing to a household with an annual budget of $30,000 and quite another thing to a household with an annual budget of $500,000. I define pre-retirement consumption as the difference between all income reported in the survey and all active savings. I use the 1992 and 1994 waves of the HRS to calculate:

(11) $c_{pre-ret} = Income_{94} - Savings_{92to94}$.

Because this measure is very noisy, I use predicted values of it in the analysis instead of actual values. I estimate a regression of log consumption on a spline of household income in 1994 and age, among households with no retired members in 1994. The parameter estimates are used to predict consumption for the sample of individuals in 1998.

Using these predicted pre-retirement consumption measures to normalize retirement resources, I create two sets of variables for the analysis that follows: (1) the ratio of sustainable retirement consumption based on expected wealth, to pre-retirement consumption, and (2) the shock to sustainable retirement consumption due to wealth shocks, as a percent of pre-retirement consumption.

[18] Bequests are excluded from the calculation of sustainable consumption for simplicity. This results in a biased estimate of baseline sustainable consumption shocks to it (the direction depends on the bequest behavior) but it does not effect the interpretation of my results. The estimates discussed below are measures of the effect on retirement probability of a wealth shock that would allow sustainable consumption to increase by 100% of pre-retirement consumption (to double). Whether or not the shock is actually consumed or bequeathed is not an issue.

I estimate the following linear probability model of retirement transitions between 1996 and 1998 among respondents in the labor force in 1996:

$$(12)\ R_i = \alpha + \beta_1 ' X_i + \beta_2 \left[\frac{{}_{92}C_i}{\widehat{C}_{pre-ret,i}} \right] + \beta_3 \left[\frac{{}_{92}\Delta_{94}C_i}{\widehat{C}_{pre-ret,i}} \right] + \beta_4 \left[\frac{{}_{94}\Delta_{96}C_i}{\widehat{C}_{pre-ret,i}} \right] + \beta_5 \left[\frac{{}_{96}\Delta_{96}C_i}{\widehat{C}_{pre-ret,i}} \right] + \varepsilon_i$$

R=1 if the respondent reports being retired in 1998. *X* is a vector of demographic characteristics that may independently affect retirement timing. It includes the following variables for characteristics of the respondent in 1998: dummy variables for single year of age, years of education, being married, fair or poor health, Black race, and Hispanic ethnicity. It also includes the following variables whose values are taken from the baseline interview in 1992: years of work experience, log earnings, dummy variables for the industry of their longest held job, and asset ownership -- DC pension, DB pension, a home, real wealth (real estate, automobiles, etc), stocks, bonds, or IRAs.

${}_{92}C_i$ is the 1992 expectation of sustainable retirement consumption, based on wealth and retirement income conditional on retirement in 1998 (*excluding* the wealth shock), and it is normalized by the pre-retirement consumption measure described above. This measure captures the expected components of wealth – wealth in 1992, savings between 1992 and 1998 and expected returns. The next three terms are the normalized annuity value of wealth shocks during the 1992-1994, 1994-1996, and 1996-1998 periods. Thus, β_3, β_4, and β_5 are the estimates of wealth effects.

As mentioned earlier, even if the level of the wealth shock is unexpected, the incidence among households is not exogenous. It could be the case that households that were planning on retiring early shifted away from stocks and thus did not have windfalls. This would bias β_3, β_4, and β_5 downward. On the other hand, if households that plan to retire early have a greater share of their wealth invested in stocks the coefficients would be biased upward. The controls for asset ownership should control for some of these effects.[19] To see how important this bias may be in estimating (12), I regress the 1992 expectation of retirement age on all of the variables used in (12), home equity, baseline non-housing wealth, and variables of the share of that wealth in stocks, IRAs, Real Estate and other non-liquid assets, and Bonds and Cash (the reference group). I find no evidence (Table 4) that portfolio choice explains any unobserved variation in retirement.

Results

Table 5 contains results for the first specification of equation 12, estimated separately for men and women. Additional control variables described above are listed at the bottom of the table. All of the coefficients on the wealth shock variables are positive, and they increase in magnitude in the lag. One should expect the finding that lagged shocks have a greater effect, since individuals have had a greater amount of time to respond to them. Among men, they are not statistically significant, and among women they are only significant when lagged.[20] Coefficients for other variables are not surprising: among men, the following are associated with a greater probability of retirement: owning a DB pension, IRA, home, or bonds; fair or poor health; retiree health insurance coverage,[21] and years of experience. Higher earnings and owning a business or real estate are associated with a lower probability of retirement. Among women, owning a DB pension, retiree health insurance coverage, and fair or poor health are associated with greater retirement probabilities.

Next, I examine whether individuals respond differently to different components of wealth. Table 6 has estimates where I disaggregate wealth shocks in each period into their three components: stocks, IRAs, and DC pensions. The table only reports estimates for wealth variables, although the same

[19] Coronado and Perozek (2001) find that asset holdings are correlated with individual expectations of retirement age. However, they find no evidence that changes in asset values over time are correlated with baseline expectations of retirement age. Thus, indicators of portfolio allocation at baseline should pick up some of the endogenous variation in retirement planning.
[20] A test for joint significance reveals that the coefficients are jointly not significantly different from zero. I also estimate the model with the wealth shocks aggregated over the time period 1992-1998. The coefficient is similar to that on the three periods disaggregated but it is also not significant.
[21] There is evidence that access to health insurance plays a large role in the decision to retire early (Gruber and Madrian, 1994).

control variables are included in the model. This disaggregation suggests different types of wealth have differing effects on retirement probabilities, and these effects differ by gender. Unexpected capital gains in stocks during the 1992-1994 period substantially increase the probability of retirement for women. Male retirement probabilities increase with capital gains in IRAs -- this is statistically significant for gains made between 1994 and 1996 and 1996 and 1998, but not for capital gains between 1992 and 1994.

The table also reveals a troubling result: although greater IRA balances are associated with a significantly greater probability of retirement for men, capital gains in DC balances are associated with a reduced probability of retirement for both men and women. IRA balances and DC pension balances should not theoretically have differential effects on retirement timing. However, because individuals with DC pensions often "roll over" their balances into an IRA when they retire, the regression may be picking up a spurious relationship. However, this is unlikely to be driving the result for several reasons. First, because the sample is made up of individuals working in 1996, lagged capital gains should not be capturing portfolio changes that occurred post retirement. Secondly, I have explicitly purged any reported active saving or dissaving of assets from the wealth shock measures. An alternative explanation for the differing results is that IRA balances may be reported with more accuracy than DC pension balances in the HRS.[22] During the interview, IRA balances are reported in the same section as other assets. For the most part, the questions on DC pension balances are in a different section of the interview. Most importantly, unfolding brackets are used for item non-response in the wealth section of the survey, but are not used with the pension questions. These brackets have been found to improve missing data imputations substantially (Juster and Smith, 1997), and could account for the difference between the quality of the IRA data and DC pension data.

[22] A number of papers (Gustman and Steinmeier (2001), and Engelhardt (2001)) have discussed the reporting error in the self-reported pension data.

For this reason I estimate equation 12 with predicted values of capital gains in DC pensions.[23] I do this by applying an average rate of return for a DC pension plan, for a given two-year period, net of the historical rate of return. Thus, unexpected capital gains for the 1996 to 1998 period are equal to the household's DC balance in 1996 multiplied by the average unexpected return in a DC plan. To do this I assume the average DC plan has half of its balances invested in equity – in the S&P 500. By using this method, I lose a lot of the variation in returns, but I lose a great deal of the measurement error as well. Results for wealth shock variables are in Table 7.

The top panel has estimates when the shocks are aggregated. These estimates can be compared to the estimates in Table 5 of aggregate wealth shocks. When projected values are used for DC pension gains, the coefficient estimates for wealth effects for men go up substantially (from 0.010-0.032 to 0.069-0.093) and are statistically significant. The estimates for women do not change much. The bottom panel has estimates when wealth effects are allowed to differ by asset type. These estimates can be compared to those in Table 6. When projected DC gains are used, capital gains in DC pensions no longer have the strong negative relationship with retirement that is observed in Table 6. In fact, capital gains between 1996 and 1998 reverse in sign from negative to positive for men. The differences in coefficients on disaggregated and aggregated wealth effects suggest that noise in DC data made it difficult to identify wealth effects that appear when this noise is reduced.

It is important to note that these estimates are of a pure wealth effect. Individuals are assumed to secure the value of their wealth by annuitizing their wealth. In reality, it is possible that individuals respond to windfalls in a variety of ways. Because they may perceive greater variance in returns in the future, they may actually feel worse off because of the stock market run up. Access to fair annuity markets, through the private market and through the family, should limit this sort of negative reaction.

[23] Others have tried to make use of the rich information in the self-reported DC pension data, by purging it of some of its error. In his study on the effect of 401(k) participation on savings, Engelhardt (2001) concludes that his most reliable estimates are neither those using firm reported data nor self-reported data, but those using imputed measures he created combining alternative sources of information.

The magnitudes of the coefficient estimates are easily interpretable. I discuss these for the preferred estimates in Table 7 of gains aggregated across assets, when using imputed DC gains. The retirement transition rate between 1996 and 1998 was 17.7 percent among men and 15.3 percent among women. Because the shocks are normalized – measured as the shock to sustainable retirement consumption assuming retirement in 1998, as a fraction of pre-retirement consumption, the coefficients are easily converted to elasticities. A shock that would allow post-retirement consumption to double increases the probability of retirement for men by seven to nine percentage points, resulting in elasticities of 39 to 52 percent.[24] For women, the effect of such a shock ranges from zero to 18 percentage points resulting in an elasticity ranging from zero for the recent shock, to 35 percent for the lagged shock, and over one for the second lag. When wealth effects are allowed to vary by asset type, they are found to be very different by asset type. Among men, unexpected capital gains in IRAs have the strongest effect, although stocks and DC pensions are also significant, and among women only stocks have a significant effect.

What effect did the stock market run up have on aggregate retirement transitions in the U.S.? Multiplication of the coefficient estimates with the mean values of the wealth shock variables gives such an estimate. Among men, unexpected wealth gains from 1994 to 1998 account for 0.3 percentage points of retirement transitions. This is just two percent of the mean transition rate of 17.7 percent. Among women, unexpected wealth gains account for 0.05 percentage points of retirement transitions, or 0.3 percent of the overall transition rate of 15 percent. The small aggregate effects are reflecting that although the data finds quite large wealth effects, because many individuals have negligible wealth gains over the period, the aggregate effect is quite small.

Retirement responses to wealth shocks may be non-linear. For example, capital gains may have to exceed a certain threshold before an individual adjust their behavior. Tests for such non-linearities are presented in Table 8. The first panel, which presents results when quadratic terms for wealth shocks are

[24] This translates into a marginal propensity to earn ranging from -0.096 to -0.13, meaning that on average, $1 in unexpected capital gains reduces earnings by about 10 cents.

included, provides some evidence that wealth effects diminish slightly as the shock gets bigger. However, this is only significant for men for wealth shocks between 1994 and 1996. The second panel presents coefficient estimates for dummy variables equal to one if an individual experienced a positive wealth shock allowing retirement consumption to increase by less than 10 percent or greater than 10 percent of current consumption, where the reference group includes individuals who experienced a zero or negative shock. These results suggest that small shocks affect retirement timing less than large shocks.

Table 9 provides additional evidence that wealth effects are larger for those with greater wealth than those with little or no wealth. The first four columns show mean wealth and shocks by these deciles. They are followed by the mean probability of retirement by decile of wealth among men. The first column lists probabilities predicted by the model when observed shocks are used. The second column contains the mean probability of retirement predicted by the same coefficients but when setting the sample's wealth shocks to zero. The difference, in the third column, shows that the effect of wealth on retirement probability does vary by wealth level. There is virtually no effect for most of the sample. However, men in the 9th decile had a 1 percentage point increase in retirement probability due to wealth shocks and men in the top decile had about a three percentage point increase.

One limitation of the data currently available is that we have not yet observed all individuals transition into retirement. If individuals who have received unexpected capital gains have decided to retire earlier than they otherwise would have, say in 2000 instead of in 2002, the analysis just presented would be an underestimate of wealth effects because the data currently available reveals no response. Fortunately, the HRS asks individuals when they plan to retire. I use responses to these questions in three different specifications. First, I estimate the change in (expected) retirement age between 1992 and 1998 as a function of wealth shocks. The change in retirement age is defined as the difference in the age the individual reported they planned to retire at baseline and their actual retirement age if they retired by 1998. If they have not retired by 1998, the change is defined as the difference between the baseline and 1998 expected age of retirement. Second, I estimate the 1998 expected (or realized) retirement age as a function of capital gains. Third, I estimate a regression of actual retirement transition while controlling for the 1992 expected age of retirement.

The first panel of Table 10 shows no evidence that individuals change their planned retirement age in response to wealth shocks. A limitation of the HRS questions on expected retirement age is that many respondents were not asked, or chose not to respond to this question. If non-response is correlated with ones retirement plans, the result would be biased. To minimize this bias, I estimate a regression of the 1998 expected retirement age on capital gains. This value equals an individual's actual retirement age if they have retired by 1998, or the expected retirement age they report in 1998. I control for the 1992 or 1994 expected age of retirement and include a dummy variable equal to one if the respondent did not report a "baseline" retirement expectation. The results, presented in the second panel in Table 10, suggest that as of 1998, unexpected capital gains are associated with a younger age of retirement, even when controlling for the expected retirement age before the bull market. All the wealth shock variables have negative coefficients, although only the coefficient on capital gains between 1994 and 1996 for men is statistically significant at the 5 percent level. It suggests that a gain that would allow retirement consumption to double results in earlier retirement by one and a quarter years.

As a robustness check, I rerun the model of actual retirement by 1998 as a function of capital gains (presented in Table 7), while controlling for one's baseline expected retirement age. Again, I include a dummy variable equal to one if the respondent did not report a baseline retirement expectation. The results presented in the third panel of Table 10 show that the estimated wealth effect is robust when controlling for retirement plans. This finding should increase confidence that the estimated wealth effect is not due to a possible relationship between plans to retire early and large capital gains.

Although the elasticities discussed for the for men are in the range of estimates in the literature, the wide variation in coefficients and significance across time, assets, and gender could be interpreted as a sign that there is a great deal of measurement error in the data. Some of the large coefficients on disaggregated wealth shocks – many greater than the mean of the dependent variable, suggest that the distribution of wealth changes may be compressed in the data. This motivates an alternative test of wealth effects, whose identification does not rely on any measures of savings or wealth. I present this method and results in the next section.

Difference-in-Differences Test of Wealth Effects

In this section, I present an alternative test of wealth effects. It involves identifying a treatment group that experienced positive wealth shocks and a control group that did not. In the context of this paper, I will use a treatment group whose wealth rose unexpectedly as a result of the stock market, and a control group whose wealth did not. One of the benefits of this approach is that it avoids issues of measurement error in wealth data, because identification does not rely on actual measures of wealth. In addition, the difference-in-differences avoids the problem that the incidence of the shocks may not be exogenous.

An interesting source of variation is across pension plan type. Workers with DC plans, whose pension value depends directly on the stock market, should have experienced, on average, large unexpected increases in their pension wealth in the nineties, whereas workers with only DB plans, or without any pension should have experienced no unexpected increases in their pension wealth. Although the data in the HRS on DC pensions does not seem to capture this (Table 2), aggregate data does. Figure 1.3 plots aggregate DC balances between 1988 and 1999 and shows that balances grew substantially during the late nineties. The graph shows that the difference between actual balances in 1999 and the balances one would have expected based on the annual rate of change between 1988 and 1994 is $579 billion or 27 percent of actual 1999 balances. Because the number of individuals with DC plans has been growing, some of the increase in balances must be due to higher coverage and contributions, but some of it is due to capital gains. This suggests that those with DC pensions, especially those with a significant portion of their balances invested in stocks, had significant unexpected capital gains. As a result, if wealth affects retirement timing, we would expect to see different patterns of retirement among workers with DC plans compared to workers without DC plans. In particular, in the late nineties, we would expect to see workers with DC plans retiring earlier than others.

This strategy assumes that once controlling for observable characteristics of an individual, a worker's pension plan type is exogenous – i.e. not influenced by the nineties economy and not correlated with any other characteristics that would affect retirement timing. Because workers' choice of pension plan is generally determined with their choice of job, it is unlikely that the pension type of workers close to retirement in the nineties was influenced by events in those years. Unfortunately, however, pension coverage is not randomly assigned. First, the likelihood of coverage from any type of pension increases with age, tenure, earnings, and firm size. Workers' job decisions likely reflect their preferences over salary, other forms of compensation, and mobility. Indeed, a large literature exists describing the incentives of firms to offer pensions and workers' job decisions (Clark and Quinn, 1999). Thus, a comparison of workers with DC plans to those with no pension would be capturing many differences other than the differential wealth shocks. However, if workers with pensions of different types are similar in other ways, individuals with DB pensions may serve as a comparison group to workers with DC pensions.[25]

However, a concern in comparing workers with DC pensions to those with DB pensions is that pension rules and accrual rates have a strong and independent effect on retirement timing. Because of the complex rules and accrual rates of DB plans, there are strong incentives to postpone retirement until a certain date, after which there are strong disincentives against continued work. In fact, there is evidence that incentives of DB plans are responsible for increases in early retirement among men (Stock and Wise, 1990). If this is the case, estimates of a nineties wealth effect based on the difference in retirement outcomes of DB and DC participants would be biased. In particular, one might find that individuals with DB plans retire earlier than individuals with DC plans, and falsely conclude that wealth has no effect on retirement timing.

[25] One example of this strategy is Stephens (1999) who uses workers with DC pensions as a comparison group to workers with DB pensions, in testing for agency theories of pension coverage.

One way to control for such differences in treatment and control groups is to look at the *difference-in-differences* in retirement rates between workers with DC pensions and those without them *before and after* the stock market boom, $(Ret_{1998}^{DC} - Ret_{1998}^{DB}) - (Ret_{1992}^{DC} - Ret_{1992}^{DB})$. This would control for time-invariant heterogeneity in retirement patterns across different types of workers, and for any time trends independent of the wealth gain that might affect retirement timing. The difference-in-differences would yield an unbiased estimate as long as there were no other unobserved changes occurring over the time period that differentially affected the retirement patterns of DC workers.

Using the HRS, I compare retirement rates of individuals with DC pensions relative to those who only have DB pensions, among individuals ages 55 to 60 in 1992 and in 1998 with some labor force participation in the 20 years prior to the survey. Because a new cohort of individuals born between 1942 and 1947 joined the study in 1998, it is possible to create mutually exclusive samples of individuals ages 55 to 60 in 1992 and 1998. The 55-60 year old respondents used in 1992 are members of the original cohort. In 1998, the 55-56 year olds are members of the new cohort, first interviewed in 1998, while the 57-60 year olds are members of the original cohort first interviewed in 1992. This analysis is looking at a greater share of "early" retirement than the analysis of household level capital gains, because of the sample constraints.

Although the HRS has administrative data on pensions for many of its initial respondents, in this paper I use the self-reported data.[26] There is evidence that individual reports of their pension plan characteristics are often inconsistent with information from administrative sources (Gustman and Steinmeier, 2001). The primary reason I use self-reported data is that the administrative pension data is not yet available for respondents who were added to the sample in 1998. In addition, it may be better to use incorrect data that reflects the individual's incorrect beliefs when studying whether individuals respond to retirement incentives or wealth shocks.

[26] Missing pension data for the 1992-1998 waves of HRS were imputed by Alan Gustman and Nahid Tabatabai. I am grateful to them for early access to the data.

Table 11 contains retirement rates among the treatment and control groups for 1992 and 1998, separately by gender. It shows that while the retirement rate among individuals with DB pensions decreased slightly over this time period, it increased for those with DC pension plans, from 7.7 to 12.8 percent for men and from 7.9 to 9.8 percent for women. The difference-in-differences is 7.8 percent and statistically significant for men, suggesting that men who experienced wealth shocks in this time period were 7.8 percentage points more likely to retire than they otherwise would have been. The estimate is much smaller for women and not significantly different from zero.

As stated earlier, this approach yields unbiased estimates as long as there were no other changes occurring over the time period that differentially affected the retirement patterns of DC workers. Because coverage by DB plans has been declining and coverage by DC plans has been increasing, it is possible that the composition of the treatment and control groups changed over the period. Much of the shift occurring in the nineties was among younger workers – i.e. new workers were much less likely to be offered coverage through a DB plan than previously. However, many companies that had DB pensions did convert policies to DC policies (Papke, 1999). If the companies that converted were ones with "weaker" DB plans, the DB plans that "survived" would be the generous ones. Thus, it is possible that the 55-60 year old workers in the 1992 DC group are workers with relatively "weak" pensions. This would bias the results toward a positive finding.

Friedberg and Webb (2000) examine characteristics of DC and DB pension holders in the 1992 HRS and find that people with different pensions are "strikingly similar." Non-pension assets and earnings are almost identical, and education levels are very close to one another. Additional evidence is seen in their retirement regressions as a function of pension type, which show that adding job characteristics such as tenure and industry does not change results. The fact that early DC holders were similar to DB holders suggests that DC holders in 1992 are *not* dominated by individuals whose weak DB pension plans had been converted. Nevertheless, I run some additional tests to see if characteristics of the treatment and control groups changed over time. I find no evidence of compositional changes in these groups during this period with respect to education or wealth.

Another concern is that assuming that households diversify their portfolios, pension type will necessarily be correlated with other wealth holdings. Because DB pensions guarantee a fixed stream of income after one retires, while the value of DC pensions fluctuates with the value of the stocks or bonds in which they are invested, individuals with DB pensions may hold riskier assets in their non-pension portfolios. Conversely, individuals who have invested in stocks through their 401(k) DC pension plan may be less likely to hold stocks independently than individuals who have DB pensions. Then, individuals with DB pensions would have greater capital gains on their non-pension portfolios than individuals with DC pensions. This would bias the coefficient estimates towards zero.

The bias, however, is likely to be small for several reasons. For several years, there has been an ongoing debate about the effect of 401(k) plans on household savings. Using the same data from the 1984, 1987, and 1991 Survey of Income and Program Participation, Poterba, Venti, and Wise (1995) found that 401(k) saving is new saving, while Engen and Gale (2000) found that 401(k) balances crowd out other saving.[27] In a recent paper using the HRS, Engelhardt (2001) finds that 401(k) saving represents new saving only if DB pension wealth is ignored as a form of wealth. When it is included, those with DC pensions do not appear to have greater savings than those without. In other words, saving in the form of DC pensions is likely substituting for saving in DB pensions, which is consistent with the assumptions necessary to use the difference-in-differences methodology I employ.[28] In addition, he does not find any significant differences in non-pension asset balances by 401(k) eligibility. Finally, simple tabulations of the HRS in 1992 and 1998 suggest that there are not substantial differences in portfolio shares (of non-pension assets) across pension type in either year.

[27] The likely cause of the differing results is that Engen and Gale (2000) allows the effect of 401(k)s to vary simultaneously over both time and earnings, while Poterba, Venti and Wise (1995) does not include this interaction. Bernheim (1999) provides a detailed explanation of the differences in the assumptions and resulting differences in estimated effects between the two approaches.

[28] The increased wealth observed in the SIPP by Poterba, Venti, and Wise (1995) could entirely be due to the exclusion of DB pension wealth. Engelhardt's (2001) results suggest that this could be the case.

Regression-Based Difference-in-Differences

To see if the difference-in-differences estimate is a result of compositional changes other than the wealth shock, I use a regression framework to estimate the effect while controlling for observable characteristics of the individual and their current or former employment characteristics. I estimate the following linear probability model of retirement, using the pooled cross section of individuals ages 55 to 60 in 1992 and 1998:

(13) $R_{it} = \alpha + \beta_1 ' X_{it} + \beta_2 Y1998_{it} + \beta_3 DC_{it} + \beta_4 DC_{it} \times Y1998_{it} + \varepsilon_{it}$

Recall that the pooled cross section is made up two mutually exclusive groups of individuals R=1 if the individual reports being retired at the time of the interview. *Y1998* is a dummy variable that equals one if the respondent is observed in 1998. Its coefficient captures any business cycle effects or trends that do not differentially affect individuals by pension plan. *DC* equals one if the respondent has a DC pension, and equals zero if the respondent only has a DB pension. Its coefficient represents any time invariant differences in retirement rates between individuals with different types of pension plans. β_4 is the difference-in-differences estimate of a wealth effect. Finally X is a vector of individual characteristics that may affect retirement timing and be correlated with pension plan type. These are: single year of age dummy variables, marital status of married, employer-provided retiree health benefits, fair or poor health, Black race, Hispanic ethnicity, education, and 13 different industry groupings.

Results

Summary statistics for these variables, by gender and year are in Table 12. Samples in 1998 are slightly smaller than those in 1992. The samples look similar between the two years in most ways. The average age in the samples, which are restricted to individuals ages 55 to 60, is 57. About 80 percent of the men are married in both years, while the rate among women drops from 71 percent to 65 percent. About 15 percent of men and a slightly higher proportion of women report being in fair or poor health. About 8 to 10 percent of the sample is of Black race and the percent Hispanic ranges from 5 to 7 percent. For both men and women, educational attainment is slightly higher in 1998 than it is in 1992. The percent working and percent retired is roughly the same in both years.

Pension coverage changes in several ways over the six-year period. The percent of women without a pension decreased from 43 to 36 percent, while non-coverage among men fell from 23 percent to 19 percent. Over this period although the percent of individuals with DC coverage only stayed roughly constant, overall coverage by DC pensions (DC only plus DC and DB) increased – from 38 percent to 55 percent among men and from 25 percent to 41percent among women. Coverage by DB plans only decreased over the period, but again the percent of individuals with either DB only or combination plans increased. Comparisons across cohorts do not show great differences in industry; noticeable differences are slight declines in manufacturing among men and in retail among women.

Regression results in Table 13 show that the difference-in-differences estimate of a wealth effect is robust to the inclusion of covariates. Men who likely had windfalls – those with DC pensions in 1998, were 6.7 percentage points more likely to be retired. Although the estimate for women is not statistically significant, the inclusion of covariates increases it from 2.9 to 4.2 percent. A comparison of the estimate for men to the difference-in-differences estimate in Table 11 reveals that heterogeneity in demographic and job characteristics included in the model (or any unobserved heterogeneity correlated with any of these 20 variables) account for only 14 percent of the original estimate.

To get a sense of the magnitude of the difference-in-differences wealth effect estimate, we can reformulate the framework as a two-step relationship between pension type, wealth, and retirement timing. Retirement is a function of wealth:

(14) $R_{it} = f(wealth_{it})$

where wealth at time *t* is partly due to endogenous savings behavior. Some exogenous portion of wealth can be identified through the interaction of pension plan type and year, due to the natural experiment created by the stock market run-up:

(15) $W = f(DB\ Pension, DC\ Pension, Year)$

Thus, interacting the differences in wealth by pension plan type with the pension plan dummy variables in (13) should yield estimates of the relationship between a dollar of wealth and retirement probability. Table 14 presents these estimates.

I will focus on estimates for men, since the difference-in-differences estimate for women was not statistically significant. First, note that because the variation on which the identification is based has not changed, other than the coefficients on the pension variables, no coefficient estimates have changed. The coefficient estimate of $DC \times Y1998$ indicates that an exogenous increase of $1,000 increases the probability of retirement by about 0.04 percentage points. Thus an exogenous shock of $50,000 would increase the probability of retirement by 1.9 percentage points.

Discussion

The results from the two different identification strategies presented in this paper provide some evidence consistent with the theoretical expectations of wealth effects. Unexpected capital gains, calculated from panel data on wealth and savings are found to increase retirement probability, but the effects are not consistent across asset type or gender. Regressions using projected capital gains in DC pensions, rather than observed gains, produce results that are closer to the hypothesized effect, reflecting that reporting error contributes to the difficulty in estimating wealth effects. When wealth shocks are aggregated across assets and DC pension gains are projected, I find that a wealth shock that would allow post-retirement consumption to double increases the probability of retirement for men by seven to nine percentage points, resulting in elasticities of 0.39 to 0.52. This is in the range of elasticities of participation reported in an earlier version of Imbens et al. (1999) in their study of lottery winners, of about 0.20 to 0.50. The wealth effect is robust to the inclusion of a baseline expected retirement age as reported by the respondent.

The difference-in-differences estimates suggest that men who likely had large capital gains in their DC pensions were 6 to 7 percentage points more likely to retire. This suggests a windfall of $50,000 would increase the probability of early retirement – retirement between the ages of 55 and 60, by 1.9 percentage points. This magnitude is very close to that suggested by the Holtz-Eakin et al. (1993) study of heirs, where they report an inheritance of $350,000 reduces labor supply by 12 percentage points (equivalent to a reduction of 1.7 percentage points for an inheritance of $50,000). My difference-in-difference estimates may understate the true behavioral response because of the unobserved heterogeneity within DC pensions. Because those individuals with DC pensions who did not have any of their account balance invested in the stock market should not respond to the stock market run up, the difference-in-differences estimates would be diluted. Nevertheless, the results presented here are robust to the inclusion of many covariates including 13 industry dummy variables, making it hard to believe that the result is driven by changes in the composition of workers with jobs covered by different types of pension plans.

These findings have several implications for the study of retirement behavior. Many papers in the 80s and 90s studied the effects of DB pension characteristics and the trend towards earlier retirement. Friedberg and Webb (2000) show that this trend may be reversing slightly as DC plans become more common and DB plans less common. This is because DC plans have smooth accrual and thus no work disincentives. Despite the lack of non-linearities in accrual, DC pension plans may have their own effect on retirement rates. The difference-in-differences results of this paper suggest that the effect may be a simple wealth effect. Thus, it may be too early to conclude that the shift to DC pensions will necessarily lead to increases in the age of retirement.

Evidence of wealth effects on retirement should be of increasing relevance as a greater share of retirement income flows from risky assets in 401(k) plans and IRAs. Allowing households to invest a share of their Social Security wealth in assets of their choice will increase this resource uncertainty further. Thus, evidence of wealth effects and the simple existence of large wealth shocks have implications for public policy and proposals to invest Social Security wealth in risky assets. Evaluations of such proposals must take into account the response of individual retirement timing to fluctuations in the value of their retirement portfolios. If individuals retire earlier in response to windfalls, they will contribute to Social Security revenues for fewer years and budgetary analyses of such proposals should account for that. The micro data results suggest that such costs are likely to be small. While the estimated wealth effects are large, the implications for aggregate retirement transitions are negligible since so many people were not recipients of large windfalls. However, because individual Social Security accounts would likely increase the number of households with equity investments, the effect of the stock market on aggregate retirement rates should increase.

While the shocks in the nineties were positive wealth shocks, many individuals recently experienced negative wealth shocks as the stock market tumbled. If workers have well behaved utility functions, such that they respond to gains and losses symmetrically, the results from the micro data suggest the effect of the 18.8 percent decline in the market this year, as measured by the Standard and Poor's 500 index, will lead to a 1.3 percentage point decrease in retirement flows in the next few years. The downturn in the market not only affects current workers who are planning their retirement, but also retired workers whose consumption depends on the balances in their 401(k) plans and IRAs. If workers responded to gains in the nineties by retiring early as the results in this paper find, how are they responding to huge pension wealth losses now that they are retired? Data collected in recent years should provide information on how these early retirees are adjusting to negative wealth shocks.

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Table 1
Cross-Sectional Evidence of Relationship Between Wealth and Retirement, by Year
(among men born between 1931 and 1941)

	Mean Wealth		Regression of Retirement on Wealth and Controls	
	Not Retired	Retired	Coefficient on Wealth (in thousands)	P-Value
1998	434,555	416,160	0.000002	0.56
1996	342,555	325,857	-0.000011	0.21
1994	307,838	339,362	0.000003	0.32
1992	290,790	368,147	0.000012	0.148

Notes: Wealth is in $1992 and includes all housing, business, and liquid wealth including IRAs and DC pension balances.
Regressions include controls for industry of longest held job, years of exp. in 92, age, race, ethnicity, health status, martial status, years of completed education.

Table 2

Wealth levels and Unexpected Capital Gains in HRS, by Year

(among individuals born between 1931 and 1941, present in all waves)

	Men				**Women**			
	1992	**1994**	**1996**	**1998**	**1992**	**1994**	**1996**	**1998**
Total Wealth								
Level	312,216	313,614	378,163	476,699	217,426	217,332	235,445	306,168
	(689,972)	(663,880)	(1,040,168)	(2,189,068)	(527,539)	(434,705)	(602,003)	(1,889,219)
Unex. Capital Gain		-50,136	23,718	46,543		-29,420	-3,613	39,763
		(612,249)	(764,870)	(1,476,727)		(453,696)	(417,397)	(980,609)
Housing								
Level	73,724	77,893	79,937	100,528	71,504	69,448	67,975	75,719
	(128,674)	(184,803)	(135,300)	(421,130)	(147,494)	(89,940)	(96,179)	(96,943)
Unex. Capital Gain		9,210	3,086	19,948		2,062	-804	8,731
		(186,865)	(176,585)	(400,734)		(130,848)	(65,755)	(71,936)
Stocks								
Level	19,848	27,272	46,832	60,855	16,525	18,552	26,147	41,736
	(92,255)	(149,990)	(290,474)	(390,291)	(103,944)	(68,422)	(181,341)	(331,360)
Unex. Capital Gain		203	10,789	735		-3,301	1,418	7,941
		(155,943)	(288,609)	(449,322)		(122,525)	(144,089)	(336,546)
IRAs								
Level	19,598	28,983	35,543	51,861	17,744	23,483	27,311	37,497
	(57,391)	(74,420)	(92,521)	(137,354)	(55,028)	(57,711)	(69,033)	(103,882)
Unex. Capital Gain		5,157	2,970	9,734		1,436	1,169	5,769
		(58,313)	(65,686)	(103,106)		(61,717)	(58,543)	(77,109)
DC Pensions								
Level	27,492	24,018	40,478	50,270	7,387	8,854	9,226	11,675
	(92,733)	(78,102)	(396,256)	(371,928)	(31,848)	(39,468)	(36,437)	(48,045)
Unex. Capital Gain		-11,206	10,477	-787		-1,064	-2,303	-784
		(102,082)	(366,005)	(507,530)		(50,474)	(53,637)	(57,151)

Notes: Wealth is in $1992 and includes all housing, business, and liquid wealth including IRAs and DC pension balances.

Standard Dev. in parentheses.

Table 3
Summary Statistics of Sample Used in Unexpected Capital Gains Analysis
(among individuals born between 1931 and 1941, present in all waves)

	Men (n=1837)		Women (n=1726)	
	Mean	Std. Dev.	Mean	Std. Dev.
Demographic Characteristics				
Age	60.928	2.911	60.887	2.912
Married	0.842	0.365	0.575	0.494
Health=Fair/Poor	0.186	0.389	0.177	0.382
Years of Education	12.794	3.106	12.707	2.545
Hispanic	0.069	0.254	0.062	0.241
Black	0.107	0.309	0.177	0.382
Work Characteristics				
Retired in 1998	0.177	0.382	0.153	0.360
Years of Exp.	35.851	6.380	26.876	10.509
Log earnings	10.490	0.870	9.753	0.922
Retiree Health Ins.	0.533	0.499	0.518	0.500
Industry of Longest Held Job				
Agriculture	0.050	0.217	0	
Construction	0.106	0.307	0.008	0.087
Manufacturing (durables)	0.095	0.293	0.070	0.255
Manufacturing (non-durables)	0.169	0.375	0.063	0.243
Transportation	0.088	0.283	0.037	0.190
Wholesale Trade	0.057	0.231	0.022	0.148
Retail Trade	0.090	0.286	0.141	0.348
Finance, Ins. And Real Estate	0.041	0.199	0.077	0.267
Business and Repair	0.038	0.190	0.043	0.202
Personal Services	0.011	0.104	0.072	0.259
Entertainment Services	0.007	0.084	0.013	0.115
Professional Services	0.137	0.344	0.374	0.484
Public Administration	0.060	0.237	0.042	0.200
Asset Holdings				
DC Pension	0.359	0.480	0.287	0.453
DB Pension	0.257	0.437	0.263	0.441
Home Owner	0.862	0.345	0.831	0.375
Business/Real Estate	0.404	0.491	0.321	0.467
Stocks	0.339	0.473	0.292	0.455
Bonds	0.084	0.277	0.064	0.246
IRA	0.452	0.498	0.435	0.496

Table 4

OLS Regression of Expected Retirement Age in 1992 on Portfolio Shares and Other Variables

	Men		Women	
Age	-0.73	**	-0.78	**
	(0.03)		(0.03)	
Married	0.39	*	-0.79	**
	(0.22)		(0.18)	
Health=Fair/Poor	-0.53	**	-0.59	**
	(0.24)		(0.24)	
Years of Education	0.17	**	0.02	
	(0.03)		(0.04)	
Hispanic	-0.38		0.23	
	(0.3)		(0.31)	
Black	-0.65	**	-0.61	**
	(0.24)		(0.21)	
Years of Exp.	-0.02		-0.01	
	(0.01)		(0.01)	
Log earnings	-0.23	**	0.03	
	(0.11)		(0.1)	
Retiree Health Ins.	-0.98	**	-0.91	**
	(0.17)		(0.17)	
Home Equity ($000)	-0.0028	**	0.0000	**
	(0)		(0)	
Nonhousing wealth ($000)	0.00		0.00	
	(0)		(0)	
Share in Stocks	-0.44		-0.35	
	(0.51)		(0.51)	
Share in IRA	-0.19		0.34	
	(0.44)		(0.41)	
Share in Business/Real Estate	0.37		0.43	
	(0.31)		(0.3)	
Has DC Pension	-0.07		0.25	
	(0.16)		(0.16)	
Has DB Pension	-1.39	**	-0.21	
	(0.18)		(0.17)	
Constant	2042.73	**	2043.80	**
	(1.92)		(1.97)	
R-Sq	0.35		0.39	
n	2239		1967	

Notes: Robust SE in parentheses. ** denotes statistical significance at the 5% level; * at the 10% level.

Table 5

Linear Probability Model of Effect of Aggregate Unexpected Capital Gains on Retirement Transition 1996 to 1998

(among individuals born between 1931 and 1941, present in all waves, not retired by 1996)

Dependent variable equals 1 if respondent reports being retired in 1998

	Men		Women	
Wealth Shocks=Change in Annuity value of Wealth/Pre-Ret. Consumption				
1996-1998 Wealth Shock	0.010		0.000	
	(0.017)		(0.015)	
1994-1996 Wealth Shock	0.023		0.054	*
	(0.020)		(0.031)	
1992-1994 Wealth Shock	0.032		0.167	**
	(0.038)		(0.038)	
Other Measures of Resources				
Has DC Pension	0.022		-0.001	
	(0.02)		(0.02)	
Has DB Pension	0.078	**	0.075	**
	(0.023)		(0.023)	
Homeowner	0.075	**	0.016	
	(0.02)		(0.021)	
Has Business/RE	-0.032	*	0.008	
	(0.019)		(0.019)	
Owns Stocks	0.003		0.003	
	(0.022)		(0.021)	
Owns Bonds	0.064	*	0.004	
	(0.038)		(0.039)	
Owns IRA	0.034	*	-0.004	
	(0.02)		(0.02)	
Years of Exp.	0.003	**	0.001	
	(0.001)		(0.001)	
Retiree Health Ins.	0.049	**	0.038	**
	(0.018)		(0.017)	
Log 1996 Earnings	-0.029	**	-0.010	
	(0.012)		(0.012)	
Demographic Characteristics				
Married	-0.025		0.028	
	(0.024)		(0.018)	
Fair/Poor Health	0.055	**	0.037	*
	(0.023)		(0.022)	
Years of Education	-0.001		0.003	
	(0.003)		(0.004)	
Hispanic	-0.014		0.025	
	(0.031)		(0.033)	
Black	-0.010		0.007	
	(0.028)		(0.023)	
Mean Retired	0.177		0.153	
n	1837		1726	
R-sq	0.1527		0.1572	

Notes: Robust SE in parentheses. ** denotes statistical significance at the 5% level; * at the 10% level.
Regressions contain 13 industry dummy variables, single year age dummies, and measures of sustainable retirement consumption as a % of pre-retirement consumption, based on "expected" wealth.

Table 6

Linear Probability Model of Effect of Unexpected Capital Gains, by Asset, on Retirement Transition 1996 to 1998

(among individuals born between 1931 and 1941, present in all waves, not retired by 1996)

Dependent variable equals 1 if respondent reports being retired in 1998

	Men		Women	
Wealth Shocks=Change in Annuity Value of Wealth/Pre-Ret. Consumption				
Stocks 1996-1998	0.043		-0.017	
	(0.026)		(0.017)	
Stocks 1994-1996	0.043		-0.038	
	(0.033)		(0.053)	
Stocks 1992-1994	0.073		0.262	**
	(0.066)		(0.062)	
IRA 1996-1998	0.148	**	0.055	
	(0.07)		(0.072)	
IRA 1994-1996	0.243	**	0.179	
	(0.103)		(0.11)	
IRA 1992-1994	0.152		0.052	
	(0.113)		(0.106)	
DC Pensions 1996-1998	-0.013	**	-0.147	**
	(0.006)		(0.056)	
DC Pensions 1994-1996	0.009		-0.164	*
	(0.014)		(0.095)	
DC Pensions 1992-1994	-0.014		-0.139	
	(0.046)		(0.114)	
Mean Retired	0.177		0.153	
n	1837		1726	
R-sq	0.1617		0.1647	

Notes: SE in parentheses. ** denotes statistical significance at the 5% level; * at the 10% level. Regressions contain 13 industry dummy variables, single year age dummies, demographic and wealth measures listed in Table 5, and a measure of sustainable retirement consumption as a % of pre-retirement consumption, based on "expected" wealth.

Table 7
Linear Probability Model of Effect of Unexpected Capital Gains on Retirement Transition
1996 to 1998
Alternative Method for Estimating Capital Gains in DC Pension Data
(among individuals born between 1931 and 1941, present in all waves, not retired by 1996)

Dependent variable equals 1 if respondent reports being retired in 1998

	Men		Women	
Wealth Shocks=Change in Annuity Value of Wealth/Pre-Ret. Consumption				
Aggregated Wealth Shocks				
1996-1998 Wealth Shock	0.069	**	0.002	
	(0.021)		(0.017)	
1994-1996 Wealth Shock	0.083	**	0.053	
	(0.029)		(0.044)	
1992-1994 Wealth Shock	0.093		0.179	**
	(0.061)		(0.056)	
Mean Retired	0.177		0.153	
n	1837		1726	
R-sq	0.156		0.156	
Disaggregated Wealth Shocks				
Stocks 1996-1998	0.045	*	-0.019	
	(0.025)		(0.017)	
Stocks 1994-1996	0.044		-0.045	
	(0.034)		(0.055)	
Stocks 1992-1994	0.080		0.295	**
	(0.074)		(0.067)	
IRA 1996-1998	0.156	**	0.045	
	(0.074)		(0.07)	
IRA 1994-1996	0.271	**	0.178	
	(0.11)		(0.113)	
IRA 1992-1994	0.148		0.012	
	(0.136)		(0.115)	
DC Pensions 1996-1998	0.128	**	-0.633	
	(0.056)		(0.477)	
DC Pensions 1994-1996	-0.509		0.089	
	(0.333)		(0.36)	
DC Pensions 1992-1994	0.868		0.641	
	(1.125)		(2.621)	
Mean Retired	0.177		0.153	
n	1837		1726	
R-sq	0.161		0.162	

Notes: SE in parentheses. ** denotes statistical significance at the 5% level; * at the 10% level.
Regressions contain 13 industry dummy variables, single year age dummies, demographic and wealth measures listed in Table 5, and a measure of sustainable retirement consumption as a % of pre-retirement consumption, based on "expected" wealth.

Table 8
Linear Probability Model of Effect of Unexpected Capital Gains on Retirement Transition 1996 to 1998
Nonlinear Wealth Effects
(among individuals born between 1931 and 1941, present in all waves, not retired by 1996)

Dependent variable equals 1 if respondent reports being retired in 1998

	Men		Women	
1996-1998 shock	0.050	**	0.01	
	(0.024)		(0.054)	
1996-1998 shock squared	0.002		0.00	
	0.00		(0.007)	
1994-1996 shock	0.153	**	0.11	
	(0.037)		(0.092)	
1994-1996 shock squared	-0.008	**	-0.01	
	(0.004)		(0.017)	
1992-1994 shock	0.167	**	0.27	**
	(0.071)		(0.086)	
1992-1994 shock squared	-0.012		0.01	
	(0.036)		(0.013)	
Indicators of Wealth Shocks <10% and >10%				
1996-1998 had large shock	0.084	**	0.01	
	(0.03)		(0.031)	
1996-1998 had small shock	0.005		0.00	
	(0.019)		(0.019)	
1994-1996 had large shock	0.058	*	0.09	
	(0.033)		(0.038)	
1994-1996 had small shock	0.048	**	0.03	
	(0.02)		(0.02)	
1992-1994 had large shock	0.066	*	0.12	**
	(0.04)		(0.041)	
1992-1994 had small shock	0.017		0.00	
	(0.02)		(0.021)	

Notes: SE in parentheses. ** denotes statistical significance at the 5% level; * at the 10% level.
Regressions contain 13 industry dummy variables, single year age dummies, demographic and wealth measures listed in Table 5, and a measure of sustainable retirement consumption as a % of pre-retirement consumption, based on "expected" wealth.

Table 9
Differential Wealth Effects on Retirement Probability by Wealth Decile

		Shocks to Sustainable Consumption			Predicted Probability of Retirement		
Wealth Decile	Mean Wealth	1992-1994	1994-1996	1996-1998	Actual	With Shock=0	Difference
1	-5,377	-0.002	0.005	-0.004	0.113	0.113	0.000
2	37,804	0.001	0.148	-0.194	0.181	0.182	-0.001
3	72,824	-0.005	0.000	-0.009	0.150	0.150	-0.001
4	108,117	0.003	0.005	-0.024	0.166	0.166	-0.001
5	152,223	0.009	0.004	-0.009	0.188	0.188	0.000
6	203,512	-0.003	0.002	0.014	0.176	0.175	0.000
7	271,620	0.012	-0.002	-0.002	0.201	0.199	0.001
8	396,703	0.004	0.033	0.014	0.201	0.196	0.005
9	649,024	0.035	0.025	0.057	0.190	0.180	0.010
10	2,872,348	-0.002	0.154	0.219	0.212	0.182	0.030

Note: Predicted probabilities are from logit estimates of the form presented in Table 7.

Table 10
Effect of Unexpected Capital Gains
on Retirement Expectations and Transitions 1996 to 1998
(among individuals born between 1931 and 1941, present in all waves, not retired by 1996)

	Men		Women	
Dependent variable is change in retirement age				
Measured in expectation in 1992/1994 and remeasured (as expectation, or realization) in 1998				
1996-1998 Wealth Shock	-0.157		0.38	
	(0.138)		(0.253)	
1994-1996 Wealth Shock	-0.283		0.60	
	(0.275)		(1.253)	
1992-1994 Wealth Shock	0.350		0.34	
	(0.701)		(0.819)	
n	1,129		1,077	
R-sq	0.09		0.09	
Dependent variable is updated retirement age				
Measured as the 1998 report of retirement (expected or realized) age				
Missing baseline expectation	37.52	**	28.48	**
	(2.713)		(3.465)	
Expected retirement age at baseline	0.58	**	0.44	**
	(0.042)		(0.055)	
1996-1998 Wealth Shock	-0.23		-0.26	
	(0.348)		(0.212)	
1994-1996 Wealth Shock	-1.24	**	-0.25	
	(0.581)		(0.507)	
1992-1994 Wealth Shock	-1.10		-0.42	
	(0.700)		(0.688)	
n	1,437		1,430	
R-sq	0.39		0.23	
Dependent variable equals 1 if respondent reports being retired in 1998				
Conrols for baseline expected retirement age				
1996-1998 Wealth Shock	0.07	**	0.00	
	(0.019)		(0.017)	
1994-1996 Wealth Shock	0.10	**	0.04	
	(0.027)		(0.043)	
1992-1994 Wealth Shock	0.10	*	0.21	**
	(0.058)		(0.051)	
n	1,842		1,737	
R-sq	0.19		0.17	

Notes: SE in parentheses. ** denotes statistical significance at the 5% level; * at the 10% level.
Regressions contain 13 industry dummy variables, single year age dummies, demographic and wealth measures listed in Table 5, and a measure of sustainable retirement consumption as a % of pre-retirement consumption, based on "expected" wealth.

Table 11

Difference-in-Differences in Retirement Rates by Pension Type,* 1992 and 1998

Sample includes mutually exclusive individuals 55-60 in 1992 or 1998, with some history in labor force

	Men				**Women**		
	1992	1998	Difference		1992	1998	Difference
DC	7.7%	12.8%	5.1%	DC	7.9%	9.8%	1.9%
DB only	24.8%	22.1%	-2.7%	DB only	19.2%	18.2%	-0.9%
		Diff-in-Diff	7.8%			*Diff-in-Diff*	2.9%
			(p=0.012)				(p= 0.376)

* Pension Type is based on self reported data on pension coverage at current and/or prior jobs.
An individual is classified as retired if they identify their labor market status as retired.
Weighted by HRS sample weights for 1992 and 1998.

Table 12
Summary Statistics of Sample Used in Difference-in-Differences Regressions of Retirement Rates
(among individuals 55-60 in the observed year, with some history in labor force)

	Men		Women	
	1992 (n=2138)	1998 (n=1703)	1992 (n=2117)	1998 (n=1994)
Married	0.819	0.787	0.712	0.654
	(0.385)	(0.41)	(0.453)	(0.476)
Ret. Health Ins	0.604	0.479	0.596	0.467
	(0.489)	(0.5)	(0.491)	(0.499)
Fair/Poor Health	0.141	0.146	0.153	0.188
	(0.348)	(0.353)	(0.36)	(0.391)
Black Race	0.085	0.076	0.103	0.104
	(0.279)	(0.266)	(0.304)	(0.305)
Hispanic	0.050	0.074	0.052	0.069
	(0.219)	(0.262)	(0.223)	(0.253)
Years of Education	12.672	13.059	12.423	12.807
	(3.192)	(2.946)	(2.632)	(2.595)
Age	57.435	57.256	57.407	57.180
	(1.723)	(1.72)	(1.739)	(1.709)
Working	0.855	0.875	0.735	0.743
	(0.352)	(0.331)	(0.442)	(0.437)
Self Report Retired	0.137	0.150	0.100	0.108
	(0.344)	(0.358)	(0.3)	(0.31)
DB and DC Pension	0.232	0.411	0.123	0.294
	(0.422)	(0.492)	(0.328)	(0.456)
DC Pension Only	0.152	0.148	0.163	0.150
	(0.359)	(0.355)	(0.37)	(0.357)
DB Pension Only	0.382	0.250	0.283	0.196
	(0.486)	(0.433)	(0.45)	(0.397)
No Pension	0.235	0.191	0.431	0.360
	(0.424)	(0.393)	(0.495)	(0.48)
Sample Size	2138	1703	2117	1994

Notes: Pension Type is based on self reported data on pension coverage at current and/or prior jobs.
An individual is classified as retired if they identify their labor market status as retired.
Weighted by HRS sample weights for 1992 and 1998.

Table 12 (cont)
Summary Statistics of Sample Used in Difference-in-Differences
Regressions of Retirement Rates
(among individuals 55-60 in the observed year, with some history in labor force)

	Men		Women	
	1992 (n=2138)	1998 (n=1703)	1992 (n=2117)	1998 (n=1994)
Industry of Current or Last Job				
Agriculture	0.020	0.034	0.010	0.005
Construction	0.094	0.094	0.009	0.007
Manufacturing: Non Durables	0.087	0.102	0.065	0.060
Manufacturing: Durables	0.211	0.156	0.061	0.066
Transportation	0.132	0.125	0.044	0.042
Wholesale	0.049	0.056	0.023	0.022
Retail	0.059	0.069	0.173	0.146
Finance, Ins. And Real Estate	0.044	0.048	0.077	0.084
Business and Repair Services	0.040	0.064	0.041	0.056
Personal Services	0.014	0.009	0.050	0.046
Entertainment	0.016	0.013	0.011	0.021
Professional Services	0.154	0.166	0.396	0.394
Public Administration	0.080	0.062	0.039	0.050

Notes: Weighted by HRS sample weights for 1992 and 1998.

Table 13
Difference-in-Differences Estimates of Wealth Effects:
LPM Comparing Individuals with DC Pensions to those only with DB Pensions

Dependent variable equals 1 if respondent reports being retired at the time of the survey
Sample includes mutually exclusive individuals 55-60 in 1992 or 1998, who have hads pension

	Men		Women	
Married	-0.034	*	0.044	**
	(0.018)		(0.013)	
Ret. Health Ins	0.152	**	0.117	**
	(0.013)		(0.014)	
Fair/Poor Health	0.064	**	0.037	*
	(0.022)		(0.021)	
Black Race	0.028		0.018	
	(0.024)		(0.018)	
Hispanic	0.008		-0.042	**
	(0.028)		(0.02)	
ED<High School	-0.061	**	-0.001	
	(0.02)		(0.022)	
ED>High School	0.021		0.040	**
	(0.016)		(0.017)	
Resp. observed in 1998	-0.002		-0.004	
	(0.025)		(0.025)	
Has DC Pension	-0.157	**	-0.113	**
	(0.019)		(0.021)	
DC Pension x 1998	0.067	**	0.042	
	(0.028)		(0.029)	
Constant	0.169	**	-0.035	
	(0.059)		(0.044)	
Mean Retired	0.159		0.128	
Sample Size	2,967		2,465	
R-sq	0.1376		0.0955	

Notes: SE in parentheses. ** denotes statistical significance at the 5% level; * at the 10% level.
Regressions include controls for 13 industry groups, and single year of age.
Weighted by HRS sample weights for 1992 and 1998.

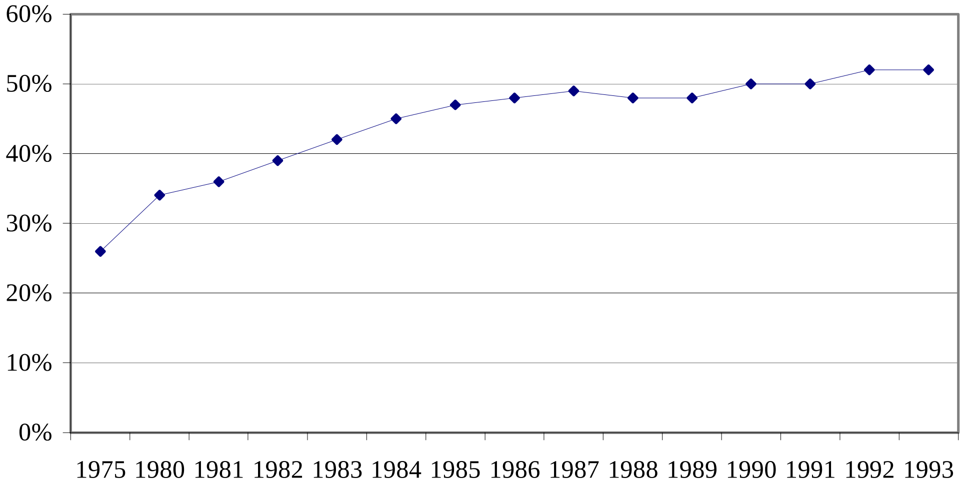
Table 14
Difference-in-Differences Estimates of Wealth Effects using Mean Wealth ($92/1000) Interactions:
LPM Comparing Individuals with DC Pensions to those only with DB Pension

Dependent variable equals 1 if respondent reports being retired at the time of the survey
Sample includes mutually exclusive individuals 55-60 in 1992 or 1998, who have or had a pension

	Men		Women	
Resp. observed in 1998	-0.000014		-0.000039	
	(0.000159)		(0.000231)	
Has DC Pension	-0.000800	**	-0.001720	**
	(0.0000962)		(0.000324)	
DC Pension x 1998	0.000388	**	0.000996	
	(0.000162)		(0.000693)	
Effect of $50,000 shock on retirement probability	0.019		0.050	
Mean Retired	0.159		0.128	
Sample Size	2,967		2,465	
R-sq	0.1376		0.0955	

Notes: SE in parentheses. ** denotes statistical significance at the 5% level; * at the 10% level.
Regressions include controls for 13 industry groups, and single year of age.
Weighted by HRS sample weights for 1992 and 1998.
Wealth is in $1000, 1992.

Figure 1.1
Percent of Individuals with Pensions with DC Pension



Source: Employee Benefit Research Institute 1997

Figure 1.2
Standard and Poor's 500 Year End Levels

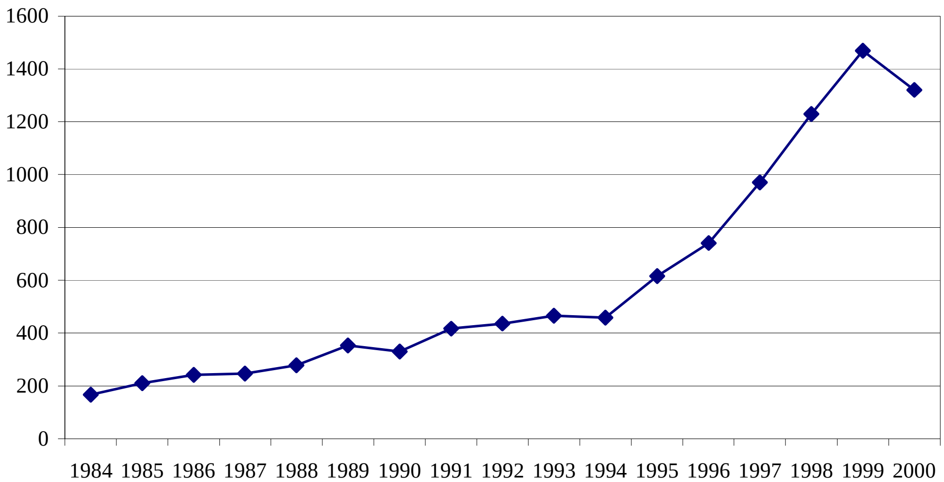


Figure 1.3
Aggregate DC Balances, billions of $1999
Actual and Extrapolated based on pre-1994 average changes